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ABSTRACT

In recent years, rural sociology has been the subject of sociological inquiries. Many of these have been highly critical, raising questions about the ontological nature of the discipline. This paper extends the tradition, providing a brief historical analysis of rural sociology's roots as both theoretical and applied and critiquing current rural sociology in a critical theory perspective. Questions about rural sociology's very being and how it could become more policy-oriented are raised. Illustrating a direction in which research and practice are more interwoven rather than merely discussed and suggesting a direction in which rural sociology could go, the paper gives examples from economics. Finally, the paper cautions rural sociologists that not to address these questions is to run the risk of creating a scientifically pure but substantively vacuous discipline. (Author/NEC)

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Bringing Rural Sociology Back In:

Critical Theory and Rural Policy*

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ABSTRACT

In recent years, rural sociology has been the subject of sociological inquiries. Many of these have been highly critical, raising questions about the ontological nature of the discipline. In this paper, we extend this tradition. We provide a brief historical analysis of rural sociology's roots as both theoretical and applied, and we then critique current rural sociology in a critical theory perspective. In this way we raise questions about rural sociology's very being and ask how it could become more policy-oriented. We discuss examples from economics which we feel suggest a direction in which rural sociology could go -- a direction in which research and practice are more interwoven rather than merely discussed. We close by cautioning rural sociologists that to not address the questions we ask is to run the risk of a scientifically pure but substantively vacuous discipline.

BRINGING RURAL SOCIOLOGY BACK IN

In the last few years, rural sociology seems to have undergone something of a revolution. Falk & Pinhey (1978, p. 556) tried to "make sense of the concept rural" and concluded that "if there is no 'rural,' can there be a 'rural sociology?'" At the same time, Picou, Wells, and Nyberg (1978) panned rural sociology for its "theoretical monism." Bealer provided his own counterpoints (see Bealer, 1978; 1979; 1981; 1983a; 1983b).[1] Subsequent years have seen a spate of articles (e.g., Newby, 1980, 1982, 1983; Friedland, 1982; Gilbert, 1982) raising in one way or another the general issue of what Americanized rural sociology is supposed to be all about. It is in that genre that the present paper has been prepared. We propose another way for rural sociologists to reinvigorate the discipline: to become more policy-relevant via partisanship on behalf of their own interests.

"I Been Down So Long It Looks Like Up."

It is relatively safe to argue that rural sociology has always been, at least in the United States, a kind of stepchild in the larger sociological community. Indeed, its very institutional roots are to be found in its separation from rather than its inclusion in general sociology. The subsequent history of rural sociology has been one of off-and-on flirting with the general discipline, but never completely capitulating to it. This is borne out by its present uneasy relationship with the American Sociological Association (ASA). Nothing bears out our "uneasy" comment better than the contentiousness of many

RSS members over the site of their annual meeting. While individual Rural Sociological Society (RSS) members have had rural sociology sections included in the ASA annual program, the RSS continues to hold its own annual meeting immediately prior to the ASA's.[2]

The early 1970s found general sociology in what Gouldner called a "crisis."[3] Newby, however, cautioned against too strict an application of the "crisis mentality," a caution with which we agree. But, regardless of what we call it, something new is afoot in rural sociology. Indeed, Newby has been at the forefront of work under the aegis of the "new rural sociology." This may comes as news to some of the RSS brethren who may justifiably ask whatever happened to (or was wrong with) the "old" rural sociology. Rather than catalog its portended ills, we will only cite Newby (1980, for the long version; 1982 and 1983 for distilled versions). It is Newby's thesis that rural sociology in America can be characterized by its "institutional" qualities, in particular its style which "determines its substance" (Newby and Buttel, 1980:4). Loosely translated, this has given rise to a unique branch of research which of necessity (by virtue of rural sociology's common location in the land grant university experiment station setting) has been scientistic, positivistic, inductive and supposedly applied. The problem with this, of course, is that it may bracket in and thereby exclude from consideration certain research problems. For example, it is not unreasonable to assume that the full range of sociological theories cannot be considered when going to the experiment station for funding since certain tenets of certain theories might be objectionable to experiment station administrators who must, after all, give the

initial approval to experiment station projects (see Lacy and Busch, 1982).

It is this type of thing that brings us to the new rural sociology: What is "new" is precisely the willingness to be somewhat more critical of the rural sociology that has gone before it and the willingness to consider sociological perspectives which might be analytically useful even if possibly meeting with disfavor from agricultural administrators. (Here, of course, we refer to Marxism as well as the "Left Weberianism" now current in Britain.) In this sense, the "new" is potentially akin to revisionist history in that new analyses could possibly yield an entirely different and, quite likely, contradictory set of findings of previously researched topics. This has one clear precedent in recent rural sociology when Summers, et al. (1976) concluded that rural industrialization was at least as much boondoggle as it was boon. At issue here are domain assumptions and a consideration of costs and benefits being derived from any particular set of actions.

The Summers work certainly had definite policy implications. This is precisely what the new rural sociology--the sociology of agriculture in particular--apparently lacks (Newby, 1982).[4] Yet one of the main rationales for a rural sociology in the first place was its policy relevance, its applied "problem-solving" orientation. In light of the current paucity of policy prescriptions, we think it may be useful to take a brief historical look at the field. Why did rural sociology arise? How did the "founding fathers" view applied research? Did they shy away from policy recommendations? We will briefly address these questions, then return to the present in order to suggest how rural sociologists might become more policy-relevant.

Early Rural Sociology: Applied and Theoretical

The first students of rural life in the United States were frequently ministers or educators, and always reformers: They sought a better rural society, which (in turn-of-the-century America) meant an improved life on the farm. They were problem-oriented, practical men (Nelson, 1969). The same holds true for the earliest rural sociologists. One significant indication of this overwhelmingly applied, nontheoretical interest is that C.J. Galpin, the founding father of the discipline in the U.S., preferred the term "rural life" (the title of his 1918 text) to "rural sociology." He did so because the latter implied merely the study of, rather than solutions to, rural problems (Nelson, 1969:33).

Furthermore, the first textbook in the field, Constructive Rural Sociology, distinguished itself by a practical orientation. It sought to "lead the way to a right policy and useful action" (Gillette, 1913:3). Gillette's second text (1922:6) asserted that the

> great business of rural sociology is, and perhaps ever will be, the attainment of a sympathetic understanding of the life of farming communities and the application to them of rational principles of social endeavor..... We may think of rural sociology as that branch of sociology which systematically studies rural communities to discover their conditions and tendencies, and to formulate principles of progress.

Rural sociology was thus seen as an applied science, and as such asked: "What shall or should be done?" It provided the basis for social reform (Gillette, 1922:7-8). There is no doubt that the original rural sociologists were committed not just to science but, in addition, to a "constructive" improvement of society.

However, this early view of the discipline did not remain unchallenged. In fact, the most influential text published between the world wars urged a sharply different ideology for rural sociology. In their Principles of Rural-Urban Sociology (1929), Sorokin and Zimmerman presented the field as a theoretical science. Probably in direct reaction to Gillette's formulation of the rural sociological tasks, Sorokin and Zimmerman (1929:vi) stated explicitly that a "sympathetic" understanding of rural life was unscientific. The point of a theoretical rural sociology was not to preach or evaluate, improve or reconstruct, provide advice or solve problems. Rather, sociologists should first describe, then explain, differences in rural and urban social words. Then, perhaps, an applied rural social technology may be called for (Sorokin and Zimmerman, 1919:8-10).

Obviously, this version of rural sociology was at odds with the discipline's original statements of purpose. Just as obviously, this later "scientistic" perspective eventually came to dominate the field. Sorokin and Zimmerman's work began the eclipse of the "Country Lifers" and the "problems" approach in rural sociology (Nelson, 1969:113). The 1930s marked the period during which the shift occurred. Hooks (1983) has recently provided an excellent analysis of this transition for rural sociology.

In the 1930s, Carl Taylor exemplified the synthesis of both earlier (and usually separate) aspects of rural sociology: applied reformism and theoretical science. He saw each dimension as being necessary to the other in order to have a fully developed discipline (Hooks, 1983:399). In contrast was the position of another prominent rural sociologist, Dwight Sanderson, who favored a politically conservative,

inductive, and "consensus" approach. Historically, Sanderson's version won out--but not due to any scientified superiority. On the contrary, Taylor's brand of engaged rural sociology died a quite political death. Large landowners in California and Mississippi were greatly offended by some studies (e.g., Goldschmidt, 1947) that Taylor had funded and supported. They were instrumental in killing off Taylor's (1940) merger of "social theory and social action."[5] The overriding point here is not to recount history but to underline the inherently politicized nature of rural sociology (Hooks, 1983). That being the case, we can now turn to a discussion of how we might begin bringing rural sociology back in--<u>from</u> practical irrelevance, <u>to</u> policy issues and debates.

Critical Theory and Policy Research

Our reading of the current rural sociology literature leads us to conclude that: (a) rarely are rural sociologists inclined to be very critical of the status quo, and (b) any policy suggestions they set forth are often timid and serve only to perpetuate what exists (albeit with some minor tinkering). That this is the case is quite easily understood. The primary explanation is derived from the realization that rural sociology occupies what many would admit is a marginal position. Like Stonequist's (1937) "Marginal man," we have our feet planted in different camps, each with its own set of normative standards. We suspect that on the one hand there is academic rural sociology, complete with some pretext of "academic freedom;" on the other hand exists the agricultural experiment station with (potentially) a somewhat different worldview. Most experiment station-based rural sociologists have some consciousness of the need to "fit in," which may,

on occasion, take the form of becoming "one of the boys." More harshly, in a kind of Stephen Fetchit fashion, we "goes along and gets along." We simply want the freedom to do our scholarly work; if we have to be somewhat accommodating to our benefactors, so what? Everyone plays the game according to some set of rules, and these just happen to be the ones that apply to us.

Of course, for most of us this does not really represent a major problem. We do not experience any real crisis in our daily lives because we feel that we are being less than completely honorable. Quite the contrary. What most of us do is to ensconce ourselves under the guise of "science." This casts all of our work in a value free stance which means that in the most objective way possible we proceed with our data collection, analysis, theorizing and, perhaps, even a policy suggestion or two. This is very much like the model outlined by James Coleman (1974). Coleman, who certainly has better credentials in the realm of social policy research than nearly any other sociologist, is very clear on his belief that policy research is to be done in a value free style. His position is widely adopted by social scientists, including rural sociologists. It is only at the stages of problem selection and advocacy of results that the social scientist may leave his "scientist" cloak behind.

The stance we wish to suggest is a radical alternative to this. Our position, akin to that outlined by Nolan, Hagan, and Hoekstra (1975), is grounded in "critical theory." Critical theorists (e.g., Habermas, 1970) maintain that the production of scientific knowledge (not to mention its application) is generally ideological, i.e., used to

legitimate a particular value position. We lay claim to a central premise: Policy directed research, as well as policy oriented researchers, can be maximally effective only if research questions are stated in the context of desired answers. In the parlance of critical theory, this means an avowed commitment to a society (a) that is based on the intrinsic worth of all individuals, and (b) that maximizes human freedom. Critical theorists argue further that the Enlightenment values of truth, justice, and freedom are necessarily interrelated. As Habermas (1971:317) puts it, the "truth of statements is linked in the last analysis to the intention of the good and true life..." This position, of course, flies in the face of the conventional wisdom that strictly separates facts and values (description and evaluation, science and critique). Critical theorists aim at precisely the supercession of these dichotomies--for without bridging that gulf, there can be no critical theory of society. The empirical and the normative must be in close and definite relation to one another in order that theory and practice might be united.

Habermas (1971:301-17) holds that "knowledge and human interests" are inextricably bound up with one another. Theoretical science and social practice are intimately related. Both traditional philosophy and modern positivism reflect contemplative views of knowledge. Both thus ignore the essential "cognitive interests" of humankind, three of which Habermas elaborates. First of all, people have an innate interest in instrumental control. We have to survive in the natural world, so we undertake to master the environment through the activity of labor. The corresponding sciences to this interest-action are the empirical-analytic. Through technical (functional, formal) rationality,

they produce predictive knowledge (the deductive-nomological model) which allows us to dominate nature. The second human interest lies in mutual understanding, the social activity of symbolic interaction. Language communication exemplifies this concern for practical normative rationality; classical political theory, for example, deals with such issues as democratic consensus and agreement-formation. The relevant sciences here are historical-hermeneutic, which do not predict but rather interpret in order to discover meaning and to organize society. These first two types of science, the empirical and historical, exist in Weber's (and the Neo-Kantians') distinction between explanation and understanding. Habermas reconstructs them in terms of universal cognitive interests, i.e., on both theoretical and practical grounds.

The third knowledge-activity derives more from Marx and Freud: the human interest in emancipation. In actual societies, both of the first two cognitive interests become frustrated. The need to control nature is extended to the domination of people, who are treated as things in violation of the human interest in free and open interaction. The predictive science model approaches society as a nature-like object when it is actually a human creation, socially constructed. This instrumental means-end view of society detracts from specifically social relations, i.e., mutual cooperation. Social science thus becomes technocratic and manipulative, ignorant of its basis in shared understanding. The result is systematically distorted communication, or ideology.

The social activity of this third cognitive interest is critical practice oriented toward liberation. The theory that informs it is critical sociology, whose knowledge is emancipatory, i.e., the critique

of ideology. Freud, for example sees neurosis as self-manipulation, and Marx views commodity-fetishism as functionally necessary to capitalist society. In the way that Freud's psychoanalytic practice seeks to conclude with a liberated person, free from inner domination, Marx wants to "cure" society from class exploitation. The primary interest of this third type of science is not prediction or interpretation except insofar as these can contribute to emancipation. That is, it is useful to interpret dreams, or to uncover the "laws" of motion of capital, in order to destroy their domination over individual and society. Seemingly natural objectivities are exposed as historical ideologies. So-called causal laws may be discovered the better to abolish them both in individual consciousness and social reality--this is the aim of critical theory. A critical social science, then, is concerned to go beyond the establishment of nomological knowledge, on to "determine when theoretical statements grasp invariant regularities of social action as such and when they express ideologically frozen relations of dependence that can in principle be transformed" (Habermas, 1971:310).

It is this "transforming" quality which is especially attractive, since it takes us beyond analysis into the realm of change itself. It does not means that the scientist him/herself must be the change agent but it does make the scientist an architect for things which could be done. Clearly, this is only made possible by the explicit recognition of the 'ideologically frozen relations of dependence.' This is a necessary although not sufficient condition for beginning the change process, and to begin this process we must have some idea of the type of change that is desired. Phrased differently, and to return to our

earlier key premise: "Policy directed research...can maximally be effective only if research questions are stated in the context of desired answers." To put some meat on the theoretical bones we have outlined thus far, we turn now to the field of economics.

The Example of Economics

A great dilemma for rural sociologists who are employed in experiment stations is that we are constantly reminded of our marginality. This may be because we are often housed jointly with agricultural economics which is usually the larger of the two departments, and budget and time and resource allocation are unequal. Most often (and most consciously, perhaps) our marginality becomes apparent when agricultural "brethren" discuss their clientele; that is, those groups to whom they cater, to whom they are important, and from whom they often receive grants. It is here, more than on any other single criterion, where rural sociology falls short--and it is this point that causes us to suffer a kind of inferiority complex, at least for us in the agricultural colleges. Almost without exception, we have no specific clientele group who will lobby our case, who will tell administrators that our work is important, who will use our results in important ways. Moreover, in most cases, no direct line can be drawn from our work to some tangible output. Rarely, if at all, do we make some enterprise more profitable or functional, or make any person or group feel better about things, or help farm people to understand their roles and to improve the quality of their daily lives. While we are admitted students of rural life, we are usually on the outside looking in; rarely are we part of the action. As Friedland (1982:605) said, we are "essentially irrelevant to what is going on in the world."

But this does need to be the case. Even in our amorphous, clienteleless state, we can make ourselves known. How? We can do it by clearly directing our work toward ends that we desire. We can do it by discarding any pretense of value freeness, at least as that term applies to positivistic, scientistic rural sociology with little thought or concern for addressing Lynd's (1939) admonition, "Knowledge for what?"

These steps are not as radical or without historical precedent as some might think. By design or by default, they regularly occur in extension rural sociology since this area is concerned first and foremost with application. Also, extension sociologists are often in regular contact with individuals who do, in effect, constitute clientele groups. Of course, our colleagues in agricultural economics find easier application of their work since it is designed to be useful--whether it be for commodity groups, finance groups, or others. But these cases, both in extension sociology and agricultural economics, are not exactly where we believe the case can best be made for rural sociologists (or even sociologists generally, for that matter). For that, we turn to the area of economics.

Economists have had a much easier time of being accepted by policy makers than have sociologists. Senior sociology colleagues have on many occasions talked about the President's Council of Economic Advisors with more than a little envy in their voices. What do members of this council know that we don't? How solid are their empirical results and theoretical explanations? Why isn't there a President's Council of Sociological Advisors or a similar group for the Secretary of Agriculture made up of rural sociologists?

Although the answers to these questions go far beyond the scope of this paper (and would be specious anyway), it seems patently obvious to us that economists are not terribly bound up in gnashing their teeth and wringing their hands over "value freeness." Instead, they admit at the outset that they are supportive of some world view, and then they proceed to document how the world works within the parameters of this world view. We will briefly illustrate only one case of this--a case, as it turns out, which will sound all too familiar.

Once President Kennedy discovered poverty in America (via Michael Harrington's The Other America), and once the U.S. embarked on its "war on poverty", a major emphasis was placed on "human resource development." Translated, this meant that individuals were equated with commodities; they could be developed and improved upon just like inanimate objects. In his presidential address to the American Economics Association, Theodore Schultz (1961) discussed the potential for developing "human capital." Learning new skills, going to school, having valuable work experiences--these were all ways of investing in oneself. The successful person was the one who could wisely invest in himself or herself and then parlay that investment into an economic payoff in the labor market. It was just like investing in stocks, except that this time the "over-the-counter sale" involved the individual, not paper.

Not too surprisingly, this kind of perspective was widely adopted--in language and in practice--and there was a rash of "manpower" training programs, all guided by one central premise: better trained people get better jobs. It is crucial to understand that this trend in

government policy was no fluke; it was part and parcel of a theoretical paradigm in mainstream economics.

But by the early 1970s, this program, and the process it helped to spawn and to justify, was being criticized. It placed too much emphasis on the individual and not enough on the structural constraints under which the individual labored (or at least wished to). It failed to recognize that race and sex, among other things, were important considerations for how one fared in the job market. Critics asked how two equally qualified individuals (but of different races or sexes) could have such sharp earnings differences? The answer was found in focusing on the structure of the economy. It could not be taken for granted that the economy was a homogeneous entity that operated on free market principles. Not everyone had equal access to all parts of the market. Even where access was relatively open, not all entrants fared equally well.

The research tradition that sprang up around this critical orientation is generally related to political economics. It is not exclusively Marxist in its intellectual roots but a good deal of Marxist thought is found there. That by itself would not make it either good or bad, better or worse than any other competing orientation. What we do find particularly attractive about it, however, is that its adherents do not make any pretense about hiding behind some veiled and objective, value free view of the world. Instead, they assault what exists and ask how it can be changed. They risk asking axiological questions and posing axiological answers. In short, values are very much a part of their research agendas--not as a special topic, but as an ongoing part of their work. Paraphrasing on ontological expression from Marx, here

is a group of researchers who are themselves in and out of their work. They are not the androgynous characters which many social scientists claim to be. They observe what goes on around them. They comment on what they like and dislike. They passionately argue for why things should be changed as they would like. In the best sense of the term, they seriously grapple with C. Wright Mills' "sociological imagination." They try to understand the individual and his/her place in the world with an eye toward improving the relationship.

Two recent examples from their literature well illustrate this. First Bluestone and Harrison's book (1982) on "deindustrializing America" is a classic case of describing what exists solely for the purpose of improving it. They carefully document major historical trends in the industrialization of America. They refute such popular myths as attributing the decline in America's industrial base to higher wages and labor difficulties blamed on unions. They document how investment decisions made with expeditiously desired results have led the U.S. industrial base into the present state. They conclude with a very clear agenda of items which must be undertaken to improve the economy. Chief among these is the need for a centrally planned economy, which sounds and probably is somewhat socialistic. They call this a "democratic reindustrialization of the American economy." Second, Bowles, Gordon, and Weiskopf (1983) have undertaken a very similar analysis of the American economy (although theirs' is richer in detail) and drawn similar conclusions. They, too, provide a point-by-point agenda for what must be done to once again make the U.S. competitive. And, like Bluestone and Harrison the importance accorded the individual

is paramount; in fact, their concluding section is titled "An Economic Bill of Rights."

Value Free Rural Sociology?

We suspect that many rural sociologists are a bit squeamish about "flying our colors" to the degree that we have suggested. The work by the political economists (cited above) is especially opinionated and polemical. There is little pretense about objectivity as we normally use that term in social science. This seems to fly in the face of a particularly sacred social science canon, harking back to Weber's (1946) classic essay on "science as a vocation." What we are suggesting is that it may be possible and desirable to work toward a merger of polemics and research.

At least since Gouldner's (1962) seminal essay on the myth of value free sociology, sociologists have recognized that their research pursuits were not without some unstated ideological trappings of one kind or another. As Kaufman (1971:398) says, the "most dangerous theorist is the one who says he has no theory." Our values must enter our work. We are human beings before we are researchers, and our humanness does not suddenly get divorced from our ontological experience once we put on our scientific cloaks. Pure objectivity, even were it possible, would have as its cost that we bore no responsibility for how our work was used. We would simply be courtesans for whomever wished to pay the bill. Market contributions would prevail as they do in other buyer-seller relationships: rural sociology would become commodified.

We are claiming that <u>if</u> there is a commitment to individual worth and human freedom, <u>then</u> our research agendas should be conducted in light of those commitments. This is not really all that radical. As we

have already indicated, much early rural sociological research was conducted with high moral and social purpose. It was done with the specific intent of helping others and improving society; it reflected what may be rural sociology's most noble historical calling. In fact, Kaufman (1971) traces the parallels between rural sociology and the "radical" sociology of the early 1970s. Although the two certainly had different styles, both radical and rural sociologists shared a problem-oriented approach to their discipline. Kaufman (1971:401) concludes that "the sociologist in his practice is a whole man." It was precisely in the denial of this "moral basis of sociological practice" that rural sociology missed the boat. In becoming so incredibly bound up in scientized rural sociology, the crucial "human touch" was lost. Yes, we study communities, soil erosion, population change, ad nausem, but to what end? Or as Lynd (1939) put it, "knowledge for what?" We are essentially re-asking his question--but with a rural twist. And we are proposing an answer: human worth and freedom.

While the Nolan et al. (1975:452) characterization of critical theory may not be perfect, we nonetheless find their major points and the application of those points to rural sociology instructive:

> First, critical theory has a distinctive and definite view of the nature of man; second, critical theory offers a unique position on the role of knowledge in the construction of theory and how such knowledge is to be obtained; and finally, critical theory offers a clear-cut set of objectives for constructing the sought after social world. Rural Sociology, it seems to us, lacks all of these distinguishing features. There is no clear-cut definition of what constitutes the nature of man; the scientific method is generally accepted without criticism; and most certainly there is no vision about what rural society ought to look like!.... Until rural sociologists have some notion of what constitutes the 'oughts' of a 'good' society there will be very little they can say about social policy.

These accusations are disturbing. The picture painted of contemporary rural sociology is anything but flattering. Worse yet, they are hardly alone in these observations. As Copp (1972) observed in his RSS Presidential address:

> In my opinion, we know less about contemporary rural society in 1972 that we knew....in the 1940s....If most of the research which rural sociologists were doing in 1969 and 1970 were to have somehow disappeared, the world would have noticed little loss.

Or Dewey's (1960) pithy statement about the rural-urban continum: "real but relatively unimportant." Or Friedland's (1982:605) Marxist charge: "Until rural sociologists.....ca work....not simply to interpret the world but to change it....rural sociology will remain essentially irrelevant to what is going on in the world."

Where From Here?

As the British say, "What to do?" In Alvin Gouldner's (1970) terms, we are calling for a "reflexive" rural sociology--one that assesses itself and realizes the inherent difficulty in separating the researcher from his/her research. We are encouraging, as Kaufman puts it, the "whole man" (person), not the fragmented role-player which we often feel compelled to be.

If followed, our ideologically-sensitive, policy-oriented rural sociologist has before him/herself the task of creating clientele. All that we have said thus far is risky only on intellectual grounds. But courting particular constituencies is another matter--it is politically risky. It involves choosing sides, casting your lot in one way rather than another. It means that the costs to an agribusiness approach, to take but one example, must be weighed as well as the benefits. That the

creation of "human resources" (an individual matter) without equal attention to the creation of jobs (a structural matter) is simply unacceptable. That community development means just that--community development--as opposed to "development," whether the general community benefits or not.

Let us take the case of natural resources. "Natural resources" is implicity prefaced by the term "scarce." Indeed, the consumption of these resources at a pace which did not sufficiently allow for their replacement is directly (but not solely) responsible for the large boon in natural resources research. If resources are scarce, if they are consumed at a pace faster than they can be reproduced, then clearly some difficult consequences face possible users. Foregoing the likelihood of equitable distribution, some form of competition to gain access to and control over the resource seems inevitable. The rural sociologist enters this at a point where decisions must be made about the "use value" for one group as opposed to another. Deciding how important the resource is for any group (its "functional importance") is not just an empirically determined thing. Instead it involves values, choosing one thing over another. And it is "one thing over another," usually. Parity is more often a desire that a realized objective.

For the rural sociologist, then, choices much be made about what should be advocated. Clearly, this is a situation in which some thought must be given to the issues raised by Nolan, et al.: What type of rural society do we want? Should our commitment to a (scientific) research ethos prevent us from using our research as a tool for change? Is it wrong in science to advocate as part of one's scientific activity? Or does such advocacy transcend the bounds of science? Again, this is a

case of values. Our argument is that it may not be wise to separate the researcher from his/her research. Others, of course, will disagree.

If we take rural sociologists at their word, then their collective path and institutional identity seem likely to continue unabated. Review articles by Stokes and Miller (1975) and their forthcoming update document the positivistic side to rural sociology.[6] Concern about too much science, too little substance was apparent in the RSS Presidential Addresses of Copp (1972), Ford (1973) and Warner (1974). Copp and Ford were also concerned about public policy and (in Ford's case) public service. In all cases, there was a sense of having somehow lost sight of a grander research than currently prevails. There was an expressed need to call a "time out" and take a hard look at ourselves. To account for the good but to also ask what was not being done and to suggest new directions. It is particularly with respect to these later two issues that this paper has been developed.

What we are calling for here is a far cry from conventional rural sociology. What are calling for is nothing short of a complete overhauling of how we approach our subject matter, of how we conceptualize and subsequently execute a research project, of the mission we set out for ourselves. What we are calling for is the removal of ideological blinders. Our position asserts that it is foolish to continue separating the tasks of thought and action. If rural sociology wishes to be a participant and not simply an observer, then it must make itself known. And this cannot be done, effectively, while dancing around in some value-free guise. If we have something important to say, then we must do so in a polemical way. It must be polemical because without passion there is little reason to believe that even we put much stock in

what we say. In the absence of this, the bitter assessment of Nolan, et al.(1975:444; 452), may still be all too accurate:

> (As) long as rural sociologists allow a methodological tail to wag their research dog (as it currently seems to be the case) they will never have very much to offer in the way of social policy recommendations....If after self appraisal, one can say that he or she is satisfied with a research style that is essentially idiographic and tends to perpetuate the status quo....then so be it. At least, the image of our discipline as 'applied' and unique would be seen for what it is--a myth.

FOOTNOTES

[1]A particularly good Bealer aphorism is found in his RSS Presidential Address. Conjuring up an appropriately "rural" image, Bealer states that "Old 'chestnuts' can make good feed." Anyone who can turn a phrase like that merits some attention.

[2]Choosing the site for the RSS annual meeting is an event worth investigation by political scientists. Apparently we are going to Salt Lake City as opposed to San Francisco, on the heels of College Station over San Antonio and Blacksburg, Virginia over Washington, D.C. One can only wonder about a type of self-flagellation among the hearty souls of RSS.

[3]To be sure, the magnitude of this 'crisis' meant more to sociologists than anyone else since the larger community, academic and otherwise, probably sees sociology as little more than a diversion from more pressing matters. As the headmistress at Falk's son's school in England said to him when told of his professional identity, "I'm afraid that sociology is something of minefield in England."

[4]Friedland (personal correspondence) is quick to make the case that the sociology of agriculture he has outlined <u>is</u> by design policy oriented (for example, see Friedland, 1982), thus his view stands in contrast to that cited by Newby (1982).

[5]As we hope will be clear, at least by example, we see Taylor's sociology as an exemplar for our own view. At the recent RSS annual meeting, Ed Moe told us that Taylor understood, better than most, the

pioneering sociological work which was possible under the rubric of rural sociology.

[6]The Stokes and Miller paper, at the 1984 RSS meeting, specifically noted the continuation of this trend since 1974. This paper will be published in a special 50th anniversary issue of Rural Sociology.

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